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A robust approach to the calculation of paleostress fields from fault plane data: Reply

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WE WELCOME the opportunity to reply to Norman Fry's detailed comments and his valuable suggestions on our discussion of the robust estimation of paleostress fields from fault plane measurements. Fry's principal criticisms concern: (1) the choice of the input parameters we minimize in order to find the best LMS (least median of squares) solution, (2) our use of the Angelier *et al.* (1982) constraint equation to express the colinearity between the resolved shear stress and the fault plane striation; and directly related to this point, (3) the fact that we consider any positive shear stress, however small, as being able to initiate fault slip.

Point (1) above is strictly correct but does not (and nor do points 2 and 3) influence the conclusions reached in our paper. Fry is mainly concerned about the use of geometrically dependent parameters and, thus, about the fact that low-angle faults might pose a potential danger to the estimation of paleostress directions from such faults. At one level, his concern is justified, and, moreover, his suggestions as to which parameters to regress adds an interesting aspect to our work. The danger of low-angle faults spoiling paleostress estimations is undoubtedly high if the observations are regressed by conventional least-square techniques. However, the LMS regression estimator, with its breakdown point of up to 50%, is perfectly capable of dealing with such a situation, as it is designed to find the main trend in highly-contaminated datasets. In the context of the paleostress problem, $(n-4)/2$ (Will & Powell 1991) erroneously recorded observations can be detected, so the breakdown point is $100(n-4)/2n$, or, for $n = 25$, the breakdown point is 42%. This very high breakdown point should be more than sufficient for the vast majority of areas to be analysed reliably. However, if there is a better way of parameterizing the problem, then it should be adopted, regardless of whether the LMS estimator can cope with any shortcomings. But it is worth noting that, as with any data collection-analysis problem, the common sense of the observer is involved; for example, poorly constrained observations should be removed from a dataset before analysis, whether they are associated with nearly horizontal faults or not. In addition, a situation with a large proportion of nearly horizontal faults would be a very unusual one. In practice, probably more of a concern is the correct identification of the shear direction on the fault; however, again, the LMS estimator will generally not be sensitive to misidentifications.

Fry's second point starts with an erudite discussion on the origin of the Angelier *et al.* (1982) constraint equation, which we adopted in our work. As Fry rightly points out there are various possibilities of setting up the inverse problem. This, of course, determines the form of the constraint equation to be minimized. We most certainly agree that, depending on the requirements imposed on the constrain equation, the constraint equation adopted by us may not necessarily be always the most suitable way of defining the inverse problem. This, however, as well as Fry's discussion about the relative merits and/or shortcomings of various alternative ways of setting up the inverse problem, seems to miss the main point of our paper. The underlying logic and the main point of Will & Powell (1991) is that the reliability of paleostress calculations can be greatly improved if a robust regression estimator, such as our LMS estimator, is employed, rather than the commonly used least-squares techniques. This conclusion has little to do with the form of the constraint equation; the constraint equation used by us serves as an example, even though a highly appropriate one, as it is involved in the algorithm that has been frequently used for the calculation of paleostress directions (Angelier *et al.* 1982). We would suppose that the results of a paleostress analysis would be independent of the constraint equation for almost all datasets, although we have not comfirmed this.

Fry's third point raises an interesting point as it alludes to the incorporation of rheological parameters into the constraint equation so that the full stress tensor can be solved. It is certainly the ultimate goal of any stress analysis to obtain information about the orientations and the magnitudes of the principal stresses. Work towards the achievement of this goal is underway (e.g. Reches 1987, Célérier 1988, Angelier 1989). At the moment, however, we feel strongly that the incorporation of rheological parameters, including the shear stress 'threshold' as proposed by Fry, is not yet defensible, because these parameters and especially their changes under varying conditions of temperature, pore fluid pressure, depth of faulting, etc., are not sufficiently known. Even if, in the future, rheological properties are well known, it is not clear whether the environment of faulting can ever be well enough known to allow appropriate rheological parameters to be chosen. In addition, it should be clear that Fry's concern about the influence of predicted shear stresses of "correct sign but small magnitude" is unwarranted as the LMS regression estimator is in an excellent position to handle such situations should they arise.

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